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W. H. M. CHRISTIE, M.A., F.R.S., Vice-President, in the Chair.

William Henry Davies, 55 Great Newton Street, Liverpool;
Edward Henry Nightingale, M.A., 3 Park Row, Albert Gate, S.W.;
Cuthbert E. Peek, M.A., 7 Portland Place, W.; and
The Rev. W. H. K. Soames, M.A., Chesterton Road, Cambridge;

were balloted for and duly elected Fellows of the Society.

Note giving a Numerical Illustration of the Effects of a Percentage Change on the Sun's Mean Motion in Longitude. By E. J. Stone, M.A., F.R.S.

The following is a direct numerical proof of the changes which must result from any alteration of the values of the sidereal times at mean noon adopted for the determination of the so-called mean solar times which are used for the comparison of any existing Lunar and Planetary Tables with observations.

I shall suppose a change of one per cent. to be made in the adopted value of the Sun's mean motion in longitude. This will render the effects due to the change distinctly visible in a few days, without affecting the generality of the reasoning. The effects due to the change made in 1864 can then be inferred directly by a simple proportion.

Now, taking the Greenwich mean noon of the *Nautical Almanac*, 1883 Dec. 1, as our epoch, and expressing our time,

in terms of the adopted day we shall have, according to Le Verrier's Solar Tables

(1) Sun's mean longitude $= l = 249° \ 59' \ 55''{\cdot}22 + 3548''{\cdot}330235 \ . \ t$,

from which we find by the usual formula for the Right Ascension of the Greenwich meridian

(2) $S = 16^h \ 39^m \ 59^s{\cdot}68 + (24^h + 3^m \ 56^s{\cdot}555349)t +$ Nutation;

and let us assume that by the adoption of the value of l given in (1) we have found for the longitude of the Moon's centre of gravity

$$L = A + Nt,$$

and with the times t, deduced in the usual way from the adoption of the expression

$$S = 16^h \ 39^m \ 59^s{\cdot}68 + 3^m \ 56^s{\cdot}555349 \times t + \text{Nutation}$$

as the sidereal times at mean noon, we have found

$$A = 268° \quad \text{and} \quad N = 47436''{\cdot}72653,$$

so that

(3) $$L = 268° + 47436''{\cdot}72653 \times t.$$

But if we adopt for the Sun's mean longitude

(4) $$l = 249° \ 59' \ 55''{\cdot}22 + 3512''{\cdot}84693268 \times t',$$

we should find from the usual formula for S

(5) $S' = 16^h \ 39^m \ 59^s{\cdot}68 + (24^h + 3^m \ 54^s{\cdot}18979551) \times t' +$ Nutation,

and, although I will not assume it, from the necessary proportionality of the real motions of L, S, and l, however the units of time be changed, we should have

(6) $$L = 268° + 46962''{\cdot}3592647 \ . \ t'.$$

The values of S and S′ at tabular mean noons, when $24^h \times t = 24^h \times t' = 24^h \times$ same integer, are given in the following table under the heads of A and B respectively.

Date.		Green. Mean Noon *N.A.* A h m s	Tabular Mean Noon. B h m s
1883, Dec.	1	16 40 0·18	16 40 0·18
	2	16 43 56·74	16 43 54·37
	3	16 47 53·29	16 47 48·56
	4	16 51 49·85	16 51 42·75
	5	16 55 46·41	16 55 36·95
	6	16 59 42·96	16 59 31·13
	7	17 3 39·52	17 3 25·33
	8	17 7 36·08	17 7 19·52
	9	17 11 32·63	17 11 13·71
	10	17 15 29·19	17 15 7·90
	11	17 19 25·75	17 19 2·09

The results given under A are identical, as they should be, with those of the *Nautical Almanac*. Now, if we could assume, consistently with the conditions imposed upon us by the absolute necessity of (1), (2), and (3) representing the *same* real and independent motions before and after the change of the variable,

$$t = t'(1 - 0\,000027304334),$$

it would be found that S, which does equal

$$16^h\ 39^m\ 59^s{\cdot}68 + (24^h + 3^m\ 56^s{\cdot}555349)t + \text{Nutation},$$

would become

$$16^h\ 39^m\ 59^s{\cdot}68 + (24^h + 3^m\ 54^s{\cdot}18979551)t' + \text{Nutation},$$

or S would be identical with S′, if the substitution is one consistent with the physical conditions of our problem.

It is upon the identity of S and S′ with this small change of t that all the opposition to the acceptance of my views is in reality based; and this is the supposed practical disproof of my results.

But if S does equal S′, then, as we are concerned with the mean longitude of the *real* Sun, and not with the longitude of an arbitrary mean Sun, we ought to have $l = l'$, which is not the case.

$$\begin{aligned}\text{For } l \text{ which} &= 249^\circ\ 59'\ 55''{\cdot}22 + 3548''{\cdot}330235 \times t \\ &= 249^\circ\ 59'\ 55''{\cdot}22 + 3548''{\cdot}330235 \,.\, t'(1 - 0{\cdot}000027304334) \\ &= 249^\circ\ 59'\ 55''{\cdot}22 + 3548''{\cdot}233350 \,.\, t.\end{aligned}$$

or expressed in time $= 16^h\ 39^m\ 59^s{\cdot}68 + 3^m\ 56^s{\cdot}548890 \,.\, t'$, and consequently, therefore, we shall have

		h	m	s
Dec. 1.	R.A. of meridian at tabular mean noon	= 16	39	59·68
	Mean longitude of Sun in time at − ditto −	= 16	39	59·68
	Difference ...	= 0	0	0·00
Dec. 11.	R.A. of meridian at tabular mean noon	= 17	19	1·58
	Mean longitude of Sun at − ditto =	= 17	19	25·14
	Difference ...	= 0	0	23·56

and *as we have made the positions of the meridian agree whether we are using t times or t′ times by the relationship*

$$t = t'(1 - 0{\cdot}000027304334),$$

the adjustment of the Sun to the meridian cannot be made by shifting the position of the meridian, and must therefore be due to an erroneous allowance for the Sun's motion; a result which is

directly indicated by the differences between the values of l and l'.

Conversely, if we attempt to transfer the expressions (4) and (5) by assuming that

$$t' = t(1 + 0{\cdot}0000273),$$

into the expressions (1) and (2), it will be found that although the values of S will agree, the values of the Sun's mean longitude will disagree; and the same thing will be brought out by appealing to the longitudes of the Moon.

I have already given in my papers the explanation of these difficulties. They are due to the neglect of the unit of time T in the equation of condition by which we have eliminated ω from our theoretical investigations, on the assumption that

$$\omega = 360 \times 60 \times 60'' + n, \quad \text{instead of} \quad \frac{360 \times 60 \times 60}{T} + n.$$

The existence of the factor T leads at once to the necessity of a correction $-0^{h}{\cdot}24 \times t'$, before the results of Table B can replace those of Table A.

I am afraid that Prof. Adams has hardly sufficiently considered the fact that although the rotation of the Earth rapidly destroys angular errors and thus leads to small corrections of time when we determine the time directly from observations; yet these small corrections are not effective in removing the discordances between the tabular places and the observations. The tabular R.A. of the Moon from Hansen's Tables are now nearly a second of time too large. If we were to determine the clock errors from the tabular R.A. of the Moon instead of from our sidereal observations, we should only, therefore, deduce a correction of about a second in time to the adopted G.M.T. But this correction of time would not destroy the discordance between the tabular place of the Moon and observation. It is impossible to play fast and loose with the meridian in this way. The rapid motion of the Earth is as effective in displacing the relative positions of the meridian and any object in a given time δt as it is effective in destroying a given angular error in the time δt.

With Prof. Cayley's views I quite agree, except that, as a matter of fact, we do not at present make any distinction between the real Sun's mean longitude and that of the longitude of the mean Sun; and that we cannot therefore admit such distinctions in practice. If we did it would lead to my results.

For when we adopted with my notation a_1, we should have $n_1 = a_1 + x_1$, and when we adopted a_2 we should have $n_2 = a_2 + x_2$, and the relation between x_1 and x_2 would not be $a_2 - a_1$, since I have shown it to be impossible that $n_2 - n_1$ should equal $a_2 - a_1$, when we are dealing with the Sun's *real* motion in longitude. We cannot measure motion accurately in mean solar time so long as mean solar time is itself variable. And the corrections required for the breaks of continuity must be consistent with the founda-

tions of our theoretical investigations, that in each unit of time the Sun's mean longitude did increase some definite and assigned angle, or these breaks of continuity will destroy all existing tables and introduce apparent inequalities not due to gravitation.

Note on Professor Adams's paper in the Monthly Notices for December 1883. By E. J. Stone, M.A., F.R.S.

Professor Adams's argument respecting the small difference between Bessel's and Le Verrier's sidereal times at mean noon is sufficiently treated on pages 36 and 37. It is impossible to assume, as Professor Adams does, that the meridian can have two directions in space at the same instant of absolute time.

Professor Adams's argument, page 46, line 11, that "mean solar time is measured, not by the Sun's mean motion in longitude, as Mr. Stone's theory supposes, but by the motion of the Sun in hour angle," is one which I do not profess to understand. However the mean solar time is determined, the tabular motion in longitude must represent the true motion of the Sun in longitude, if the tables are to agree with observations. That I, therefore, do not allow an arbitrary change in the adopted values of the sidereal times at mean noon to change the tabular motion in longitude, whilst Prof. Adams confessedly allows such changes to be introduced, appears to prove that I am right and Prof. Adams is wrong.

On page 46 Professor Adams proves correctly enough that with my α_1 and α_2 he has—

$$\frac{T_1 - T_2}{T_2} = \frac{\alpha_1 - \alpha_2}{360^\circ + \alpha_2}, \quad \text{or} \quad = \frac{\delta n}{360^\circ + n}$$

with Professor Adams's notation, but it is impossible that both α_1 and α_2 or n and $n + \delta n$ can represent the real motion of the Sun in longitude in the units T_1 and T_2. (See page 34.) My result on this point is confirmed by Professor Cayley, who gets my result if the expression for the mean longitude of the sun is any continuous function. (See page 48.)

It is true that mean time can be determined from the time of the Sun's transit and the equation of time. The error in time thus determined will not, however, remove the errors of the tables in longitude. It merely expresses the time which the meridian and not the Sun takes in moving from the tabular position to the true position, and such corrections can therefore only remove the $\frac{1}{366}$th part of tabular errors due to arbitrary changes in the adopted R.A. of meridian at mean noons. Take for example:—

On 1881 September 16 the observed R.A. of the Sun on the Greenwich meridian, with error of sidereal clock found from stellar observations, was $11^h\ 40^m\ 33^s{\cdot}05$, whilst the tabular R.A.

was $11^h\ 40^m\ 33^s{\cdot}09$. The tabular longitude is therefore too large by $0''{\cdot}60$ according to this observation, when the clock errors are determined in the usual way. But if we determine the error of the sidereal clock, not from the stellar observations, but from the tabular R.A., which is the method mentioned by Professor Adams as that of the application of the equation of time, we shall find a sidereal time at noon larger by $0^s{\cdot}04$ than that found from the stellar observations. The difference of time thus found is, as Professor Adams remarks, very small; but the motion of the Sun in $0^s{\cdot}04$ is only about $0''{\cdot}00164$ in longitude; and this is the only effect produced by the change of time upon the tabular error of longitude, and is exactly the $\frac{1}{366}$th part of that required to remove the tabular error $0''{\cdot}60$.

Professor Cayley should certainly accept my results, for his conclusion is that I am right unless there are two mean Suns available (page 48, 7 lines from bottom); and Prof. Adams on page 44 distinctly states, "The only mean Sun known to astronomers is an imaginary body, which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities." I agree that this is our present usage.

In conclusion, I merely call attention to the fact that in my work I use tabular days T_1 and T_2, such that Sun's true mean longitude $=A+n_1t_1=A+n_2t_2$, where n_1 and n_2 are assigned angles, and T_1 T_2 are the corresponding adopted units. The advantage of this method is that we can correctly allow for the change of unit due to a change from n_1 to n_2 in the adopted value of the Sun's mean motion in longitude, for

$$\frac{T_1-T_2}{T_2}=\frac{n_1-n_2}{n_2}$$

is determinate. Attempts to measure motion in fallible units of the time used as our independent variable cannot but lead to error.

Remarks on Major-General Tennant's paper "On the Change in the adopted Unit of Time." By Prof. J. C. Adams, M.A., F.R.S.

The December number of the *Monthly Notices* contains a paper by Major-General Tennant in which the author arrives at conclusions which appear to him to confirm Mr. Stone's views respecting a change in the unit of mean solar time. In reality, however, those conclusions are quite consistent with my own as given in the same number of the *Monthly Notices*, and not at all with Mr. Stone's.

According to Major-General Tennant (*Monthly Notices*, p. 43), the factor by which the tabular mean motions should be multiplied in consequence of the change from Bessel's to Le Verrier's determination of the ratio of the mean solar to the sidereal day is what he calls

$$\frac{\text{Sidereal Seconds in Le Verrian Mean Day}}{\text{Sidereal Seconds in Besselian Mean Day}}.$$

Now, if n be the Sun's mean motion in a mean solar day as determined by Bessel, the sidereal seconds in a mean solar day will be

$$86400 \times \frac{360^\circ + n}{360^\circ}.$$

But if $n + \delta n$ be the Sun's mean motion in a mean solar day as determined by Le Verrier, the sidereal seconds in a mean solar day will be

$$86400 \times \frac{360^\circ + n + \delta n}{360^\circ},$$

and therefore the factor above referred to by Major-General Tennant will be

$$\frac{360^\circ + n + \delta n}{360^\circ + n} = 1 + \frac{\delta n}{360^\circ + n},$$

whereas, according to Mr. Stone's views, this factor should be

$$\frac{n + \delta n}{n} = 1 + \frac{\delta n}{n},$$

where the difference from 1 is nearly 366 times greater than it should be.

The same thing may be otherwise shown thus:—

If N denote the number of mean solar days in a mean tropical year, according to Bessel's determination, then N + 1 will be the corresponding number of sidereal days in the same interval.

Consequently, the ratio of the length of a mean solar to that of a sidereal day will be

$$\frac{N+1}{N} = 1 + \frac{1}{N}.$$

But if $N + \delta N$ denote the number of mean solar days in a mean tropical year, according to Le Verrier's determination, then $N + \delta N + 1$ will be the corresponding number of sidereal days in the same interval.

And consequently the above-mentioned ratio will become

$$\frac{N + \delta N + 1}{N + \delta N} = 1 + \frac{1}{N + \delta N}$$

Hence the ratio of the length of a mean solar to that of a sidereal day will be changed in the ratio of

$$\frac{1 + \frac{1}{N + \delta N}}{1 + \frac{1}{N}} = 1 - \frac{\delta N}{N(N+1)}, \text{ nearly,}$$

whereas, according to Mr. Stone, the ratio which measures this change would be

$$\frac{N}{N+\delta N} = 1 - \frac{\delta N}{N}, \text{ nearly,}$$

where, as before, the difference from 1 is nearly 366 times too great.

Mr. Stone's error appears to arise from his *equating* two things which are really different, and which are inconsistent with each other,—viz. Bessel's and Le Verrier's determinations of the Sun's mean motion in longitude in the same interval of time.

Major-General Tennant is wrong in supposing that solar observations are no longer employed in Observatories for the determination of mean solar time. If this were the case, it would only show that the Observatories had taken a very retrograde step, since the final test whether the mean solar times have been correctly found can only be supplied by solar observations. Whenever the mean solar times are deduced from the observed sidereal times, it is tacitly assumed that the tabular mean longitudes of the Sun which have been employed are correct; and if this is not the case, the mean solar times deduced will require a corresponding correction, which can only be found by solar observations.

Thus mean solar time may be determined with reference to a natural phenomenon,—viz. the transit of the true Sun over the meridian of a given place; and the mean solar day is the average of all the apparent solar days defined as the intervals between two successive transits, and therefore has nothing arbitrary about it. To speak of Besselian mean time and Le Verrian mean time, or of the Besselian mean solar day and the Le Verrian mean solar day, can produce nothing but confusion in our ideas of the measure of time.

Additional Note on the Change in the Unit of Time.

By Prof. A. Cayley, M.A., F.R.S.

Bessel's mean Sun is what Bessel supposed the mean Sun to be: that is, a point in the heavens the mean longitude of which is

$$l = \text{const.} + 1296027''{\cdot}618184\,.\,t + \text{term in } t^2,$$

the longitude being measured from what Bessel supposed to be the mean equinoctial point, and the unit of time being what Bessel supposed to be the Julian year of 365·25 mean solar days.

Similarly Hansen's mean Sun is what Hansen supposed the mean Sun to be: that is, a point in the heavens the mean longitude of which is

$$l = \text{const.} + 1296027''{\cdot}674055\,.\,t + \text{term in } t'^2,$$

the longitude being measured from what Hansen supposed to be the mean equinoctial point, and the unit of time being what Hansen supposed to be the Julian year of 365·25 mean solar days.

Mr. Stone, as I understand him, contends that for the same instant of time the two points are one and the same point. If so, it seems to me that Hansen's formula must be a mere analytical transformation of Bessel's, and the question would arise, Why was any change made? My contention is that the points are not one and the same point; and in support I show that, assuming them to be so, we obtain two inconsistent values for the time of the Earth's rotation. As a theoretical definition of the mean Sun, I agree that "The only mean Sun known to astronomers is an imaginary body which moves uniformly in the equator at such a rate that the difference between its Right Ascension and that of the true Sun consists wholly of periodic quantities." But in astronomy there is no such thing as absolute accuracy; and I contend that neither Bessel's mean Sun nor Hansen's mean Sun is absolutely identical with the mean Sun as thus defined.

The Belts of Saturn. By Henry Pratt.

Inquiry was made at the last meeting by Mr. Ranyard whether a narrow belt on *Saturn* had been seen by others than himself. And from the reports of the meeting in the *Observatory* and *Astronomical Register* I have no doubt he referred to a narrow belt which I also saw on several occasions in October, while making observations of the inner satellites; and again on Nov. 6 and 28, while preparing to photograph the planet, I repeated the view of the same object; but it was only to identify it as a portion of features familiar to me for many years.

For the sake of clearness, I shortly describe the whole belt system, so far as it is rendered by my With speculum of 8·15-in. aperture.

The belts of the two hemispheres are, in general arrangement, usually similar and symmetrical. On both sides of the equator similar alternations of colour and proportion are usually seen. For some distance on each side of the equator extends a zone which is usually free from any markings whatever, showing only a creamy-yellow tint. At a latitude of, roughly, about 10° occurs a strong belt, narrow, sharply defined on its equatorial side, but diffused on its polar side, and gathered in places into wispy knots and curved markings. The colour of this I always see as Vandyke brown. This narrow belt is the deepest tint of all the markings on the globe; and when our atmosphere is not perfectly translucent, it is almost or quite the only detail which can be seen. So far as I can understand, this narrow belt was the subject of discussion at the last meeting. The polar side of this brown belt and its wispy curves is softened into a narrow

zone of cream tint, which again merges into one of very pale rose-madder, which ends in a moderately sharp boundary, about latitude 25°. This is followed by a zone a few degrees in width, and of cream tint; and about latitude 40° commences a double belt, very pale rose-madder in tint, the space between them being of cream tint, and the belt highest in latitude being the broadest of the two. In turn this is followed by a zone of cream tint until the polar cap of bluish-white is reached.

Of course, in speaking of colours only faint tints are to be understood throughout the above description. I did not see the whole of these features during October, simply because I did not look for them all, but on three occasions I observed the greater portion of the detail mentioned, and by that means ensured the correct identification of the narrow belt to which Mr. Ranyard referred.

The photograph* accompanying this paper illustrates the belt system described, and is a reduced copy of a water-colour drawing which I made at my telescope on 1874 Sept. 14; and on numberless occasions since then I have re-observed the same features.

18 *Preston St., Brighton:*
1884, *Jan.* 10.

Note accompanying a Sketch of the Great Comet (b) 1882.
By Gen. G. H. Willis.

(*Extract from a letter to Mr. Stone.*)

I send you the sketch of the appearance of the comet of last year on the morning of October 19.

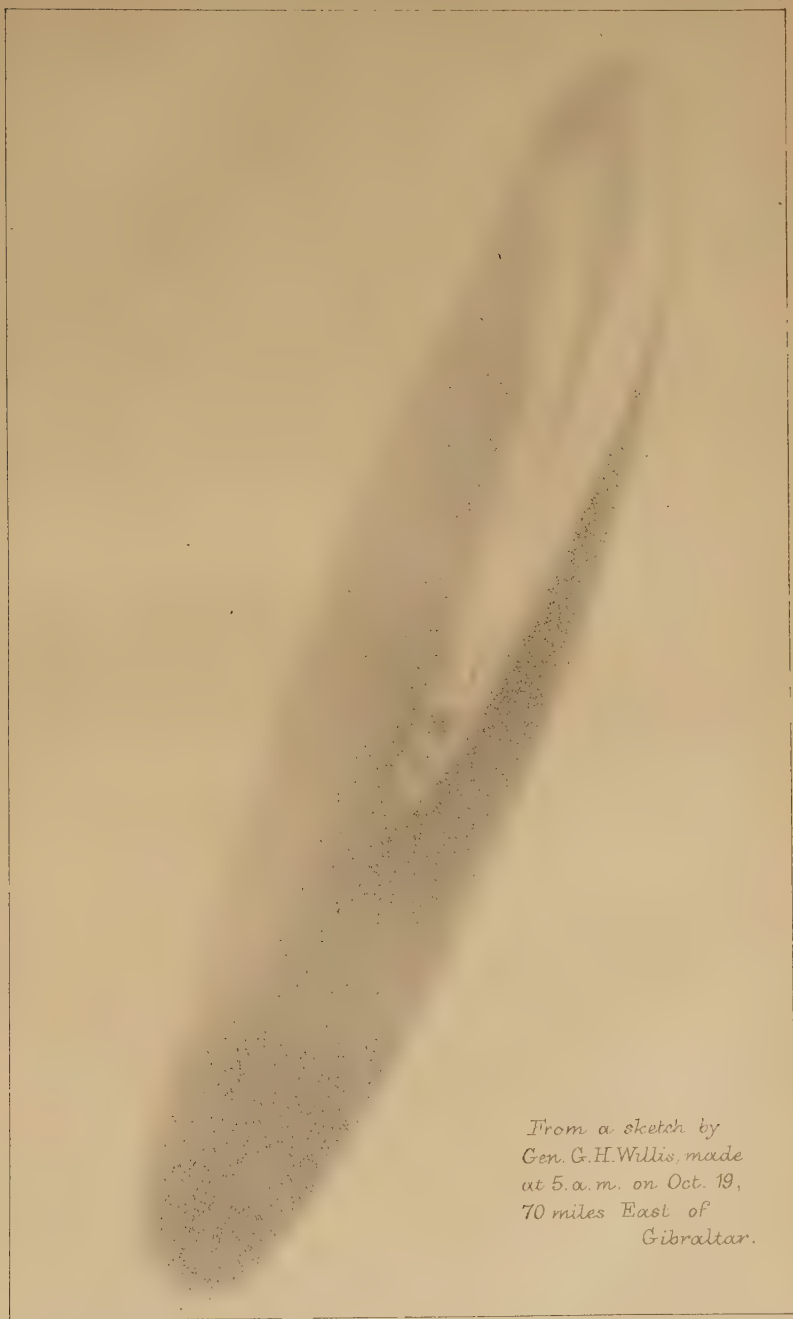
The air was extremely clear at the time, and the wind was nearly calm.

I watched the comet for some twenty minutes, until the increasing light of dawn obliterated the dark circle round it. During this time I got my field-glasses to observe with, but I found that the dark part was not so distinct through them. I then got a powerful ship's telescope, but with it the dark shade almost entirely disappeared, except on the right, or darker, side, where it remained slightly visible.

The appearance was so extremely delicate, light, and airy that it would be almost impossible to depict it on paper.

41 *Eaton Place, S.W.:*
1883, *Nov.* 15.

* The Photograph is placed in the Society's Library.



W.H. Wesley lith. West Newman & Co imp.

THE GREAT COMET (b) 1882.

Observations of Comet III. 1882 (*Barnard's*). By W. H. Finlay, B.A.

I made three observations of this comet at Aberdeen Road (South Africa), where I was stationed for the Transit of *Venus*, and as they are of later date than any I have seen published they may be useful. No micrometers were provided for the 6-inch Equatorials sent out by the Transit Committee, but I fitted a ring-micrometer (belonging to the Cape Observatory) to mine. The longitude of my station was determined by exchanges of telegraphic signals with the Cape Observatory on four nights, and the latitude by transits of stars over the prime vertical on two nights. The results are:—

$$\lambda = -1^h\ 37^m\ 15^s.6. \qquad \phi = -32^\circ\ 45'\ 57''.$$

The observations of the comet are corrected for refraction, and the diameter of the ring was determined by observations at the time. The comet presented the appearance of a small round diffused mass, slightly more condensed towards the centre, which was the point observed.

Date.		Local Sid. Time.	$d\alpha(\delta-*)$	d N.P.D. $(\delta-*)$	No. of Transits.	Comp. star.
		h m s	m s	′ ″		
1882, Dec.	3	1 16 33·8	−1 7·54	−12 19·4	6	*a*
	5	1 51 0·4	−0 15·35	+ 1 25·9	3	*b*
	7	1 54 27·4	−2 22·44	−12 33·5	7	*c*

The stars *a* and *b* have each been observed twice on the meridian at the Cape Observatory, and the results placed at my disposal by Mr. Gill. Star *c* is number 9214 of Mr. Stone's Catalogue. Their mean places for 1882·0 and corrections to date of observation are:—

Star.	Mag.	Right Ascension.	Corr.	N.P.D.	Corr.
		h m s	s	° ′ ″	″
a	8	16 29 28·42	+3·74	150 55 7·91	+0·08
b	9	16 38 36·17	+3·76	150 15 8·86	−0·65
c	6½	16 49 37·64	+3·76	149 8 30·29	−1·41

and the resulting places of the comet are:—

Date.		Greenw. M.T.	Right Ascension.	Parall. Factor.	N.P.D.	Parall. Factor.
		h m s	h m s		° ′ ″	
1882, Dec.	3	6 49 20·1	16 28 24·62	+0·086	151 7 27·4	+0·75
	5	7 15 49·2	16 38 24·58	+0·077	150 13 42·3	+0·78
	7	7 11 23·8	16 47 18·96	+0·070	149 21 3·4	+0·81

Royal Observatory, Cape of Good Hope:
1883, *Nov.* 24.

The Elements of the Orbit of Comet a, 1883 (*Brooks–Swift*).
By Robert Bryant.

The following observations of this comet were made

	App. R.A.	App. δ
	h m s	° ′ ″
1883, Mar. 3·29997 G.M.T.	0 12 23·663	+32 0 47·27

at Cambridge, England; and

		App. R.A.	App. δ
	h m s	h m s	° ′ ″
Mar. 23	7 43 50 Paris M.T.	2 51 58·5	+25 27 33
Apr. 12	8 27 36 Paris M.T.	4 19 24·2	+15 56 12

at the Paris Observatory.

These observations were corrected for the effects of parallax and aberration by means of approximate elements previously determined; and then by the variation of curtate distances parabolic elements were determined. The following elements, referred to the mean equinox 1883, are those which gave the least residuals in the middle place.

$$T = 1883, \text{Feb. } 18{\cdot}95224 \text{ G.M.T.}$$
$$\pi - \Omega = 110^\circ\ 56'\ 29''{\cdot}2$$
$$\Omega = 278^\circ\ 5'\ 59''{\cdot}5$$
$$i = 78^\circ\ 5'\ 37''{\cdot}7$$
$$\log q = 9{\cdot}8809143. \quad \text{Motion direct.}$$

These elements give for the errors of the middle place (Observation − Computation)

$$\Delta\alpha \cos\beta = +34''{\cdot}1, \qquad \Delta\beta = +74''{\cdot}0.$$

where α and β denote the geocentric longitude and latitude respectively.

The interval between the extreme observations is 40 days, and the arc of anomaly described 51°.

The Meteor Shower of Pons's Comet. By W. F. Denning.

The meteoric radiant point of Pons's comet (1812) is at 200° + 68°·5 December 6+, and the theoretical meteor speed is 37 miles per second.* The comet, however, in crossing the node, is some 20 millions of miles distant from the earth's orbit on the in-

* British Association Report on Luminous Meteors, 1875, p. 234. The radiant point computed from MM. Schulhof and Bossert's orbit (1884) is at 197°·8 + 67°·3 (*Nature*, Jan. 17, p. 273).

terior side, so that the possibility of a rencontre with any meteoric fragments belonging to it is very questionable.

In 1876, at the end of November and early part of December, I discovered a feeble star-shower proceeding from a point 6° N. of *α Draconis*. I recorded 5 of these meteors on the evening of December 8. The radiant point was at 208°+71° (*Monthly Notices*, vol. xxxvii. p. 111, Radiant No. 44). The correspondence of this shower both in date and position with the cometary radiant as above is very close, but the display, as I observed it, merely represented one of those attenuated streams which abound in every region of the heavens. The individual meteors were slow-moving, star-like bodies, devoid of streaks or trains, whereas the meteors of Pons's comet should have a velocity very nearly as great as the Perseids. It is extremely doubtful, therefore, whether this shower of Draconids, seen in 1876, can have any physical relation with the comet of Pons; though the accordance may be worth mentioning. The number of known meteoric systems encountered by the Earth is now so great that agreements of this kind carry little weight with them unless they are of a very marked and significant character.

During the last few years I have made no observations at the particular epoch of these Draconids, but on December 8, 1883, I casually saw a bright, white meteor in the fore part of *Ursa Major*, which, from its foreshortened track, obviously belonged to this radiant N. of *α Draconis*.

Bristol: 1884, *Jan.* 2.

Spectroscopic Results for the Motions of Stars in the Line of Sight obtained at the Royal Observatory, Greenwich, in the year 1883. No. VII.

(*Communicated by the Astronomer Royal.*)

The results here given are in continuation of those printed in the *Monthly Notices*, vol. xxxvi. p. 318, vol. xxxvii. p. 22, vol. xxxviii. p. 493, vol. xli. p. 109, vol. xlii. p. 230, and vol. xliii. p. 81. The observations were made with the "half-prism" spectroscope, one "half-prism" with a dispersion of about $18\frac{1}{2}°$ from A to H being used, except in a few cases of bright stars, mentioned in the remarks, where a train of two "half-prisms" with a dispersion of 80° from A to H was used. An eyepiece with a magnifying power of 14 was employed throughout.

The cylindrical lens has always been used in front of the slit as in the observations made previously to 1881. A slip of metal coated with Balmain's luminous paint inserted immediately behind the measuring pointer has been frequently employed to give a phosphorescent illumination of the field.

The observations of the Moon and of the sky spectrum have been made as a check on the general accuracy of the results.

Motions of Stars in the Line of Sight, in Miles per Second, observed with the Half-prism Spectroscope.

(+ denotes Recession; − Approach)

The initials M and N are those of Mr. Maunder and Mr. Nash respectively.

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Andromedæ.			
Aug. 23	N	2	F	−10·8	−23·1	−21·8	Spectrum bright.
Oct. 17	M	3	F	+ 3·4	−21·7	−20·9	Definition poor.
				β Cassiopeiæ.			
Nov. 16	M	3	F	+ 4·2	+ 5·9	+ 6·8	Spectrum bright & steady.
				γ Cassiopeiæ.			
Nov. 16	M	3	F	+ 2·6	+10·9	+10·5	Spectrum rather tremulous.
				α Arietis.			
Nov. 10	M	3	F	+ 3·9	+10·5	+12·7	Star-line rather faint.
20	N	2	b_1	+ 7·0	− 0·5	− 0·9	Spectrum bright but tremulous.
				β Persei.			
Nov. 16	M	3	F	0·0	−48·0	−45·9	Star-line dark.
				α Persei.			
Nov. 16	M	2	F	− 1·7	−26·1	−29·0	Star-line ill-defined.
				Aldebaran.			
Oct. 17	M	2	F	−12·8	+63·2	+65·1	Definition poor.
Nov. 10	M	2	F	− 6·3	+38·5	+36·6	
12	M	2	F	− 5·7	+32·6	+30·5	Spectrum very faint.
20	N	2	b_1	− 3·2	+34·6	+37·9	
23	M	2	b_1	− 2·2	+ 6·0	+ 7·0	Spectrum very tremulous.
Dec. 5	N	2	F	+ 1·8	+28·0	+25·3	Spectrum faint.
				Capella.			
Sept. 18	N	2	F	+16·7	+ 0·8	− 1·4	Sky hazy.
Nov. 6	N	2	F	−10·1	+40·5	+39·8	
12	M	2	F	− 8·6	+28·7	+36·1	Spectrum faint.
20	N	2	b_1	− 6·4	+44·3	+47·2	Spectrum bright & steady.
23	M	3	b_1	− 5·6	+52·2	+40·7	Definition very bad.
Dec. 5	N	2	F	− 2·0	+50·9	+42·8	Spectrum very bright.

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks
				Rigel.			
Feb. 23	M	2	F	+15·8	+22·6	+22·6	Star-line faint.
Nov. 16	M	2	F	− 5·7	+27·0	+27·1	Spectrum tremulous.
				β Tauri.			
Nov. 12	M	2	F	− 9·5	+12·9	+15·7	Spectrum faint.
				α Orionis.			
Nov. 23	M	2	b_1	− 7·8	+33·7	+43·2	Definition bad.
				β Aurigæ.			
Nov. 12	M	2	F	−10·9	+16·2	+19·2	Spectrum very faint.
				Sirius.			
Feb. 16	M	4	F	+10·3	+10·2	+16·3	Definition poor.
23	M	4	F	+11·4	− 0·7	− 1·8	Spectrum bright.
Mar. 30	M	4	F	+14·2	− 4·5	− 3·2	Two-prism train.
30	M	6	F	+14·2	−19·3	−19·6	
Nov. 16	M	4	F	−10·8	− 6·0	− 5·6	Direct comparison showed a distinct displacement towards the blue.
Dec. 5	N	2	F	− 7·0	− 1·1	+ 0·2	Star-line very broad.
				Castor.			
Feb. 16	M	3	F	+11·8	+11·4	+30·0	Definition fair.
				Procyon.			
Feb. 16	M	2	F	+10·1	+20·9	+32·1	Definition good.
23	M	2	F	+11·8	+16·1	+17·0	Star-line well seen.
Mar. 30	M	2	F	+17·2	− 3·2	− 5·0	Definition fair.
30	M	2	F	+17·2	+ 2·1	+ 2·4	Two-prism train. Spectrum too faint for this dispersion.
				Pollux.			
Feb. 16	M	2	F	+10·8	−49·3	−71·2	Definition fair.
23	M	2	F	+12·5	−33·1	−31·7	Star-line very faint.
Mar. 30	M	2	F	+17·8	−45·0	−41·2	Star-line indistinct.
May 16	M	3	F	+14·9	−66·9	−51·9	Spectrum tremulous.
Nov. 20	N	2	b_1	−15·2	−13·7	−17·5	Spectrum bright.
23	M	2	b_1	−14·6	−18·6	−24·1	Spectrum very tremulous.

Date. 1883.	No. Obs.	of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				Regulus.			
Feb. 23	M	2	F	+ 2·2	+23·7	+25·9	Star-line fairly distinct.
Mar. 30	M	2	F	+12·3	+18·1	+14·7	Definition good.
May 23	N	2	F	+17·8	− 8·4	− 8·0	Spectrum very tremulous.
23	M	2	F	+17·8	+ 5·9	+ 6·5	
31	N	1	F	+17·5	− 6·6	− 0·7	Spectrum faint.
				β Ursæ Majoris.			
July 13	M	2	F	+ 5·6	− 5·3	− 5·6	Spectrum tremulous.
				β Leonis.			
May 23	M	2	F	+16·7	+ 4·3	+ 7·6	Star-line not well defined.
				γ Ursæ Majoris.			
July 13	M	2	F	+ 7·4	+ 1·6	+ 5·3	Spectrum tremulous.
Aug. 21	N	2	F	0·0	+53·3	+55·6	Spectrum bright.
				ε Ursæ Majoris.			
July 13	M	2	F	+ 7·5	+ 2·1	+ 5·2	Definition good.
Aug. 21	N	2	F	+ 1·6	+24·8	+23·4	Star-lines very distinct.
				α Canum Venaticorum.			
June 1	M	2	F	+13·4	+11·8	+ 9·3	Spectrum faint, but star-line distinct.
				α Virginis.			
Mar. 30	M	2	F	− 3·9	− 3·5	+ 4·2	Definition good.
May 7	M	2	F	+ 7·6	−55·6	−50·5	Definition very bad.
16	M	3	F	+ 9·9	−26·3	−24·5	Spectrum bright but tremulous.
24	M	2	F	+11·8	−51·0	−52·0	Observations made with difficulty.
31	N	2	F	+13·3	−19·8	−22·7	
				ζ Ursæ Majoris.			
June 1	M	2	F	− 2·2	+25·4	+24·9	Spectrum fairly bright.
July 13	M	2	F	+ 7·8	+24·7	+27·7	Definition good.
				η Ursæ Majoris.			
May 21	M	1	F	+ 9·5	− 3·1	− 3·5	Spectrum faint.
23	M	4	F	+ 9·6	+12·4	+14·7	Spectrum bright & steady.
July 13	M	2	F	+ 9·3	−21·2	−23·3	Star-line indistinct Observations unsatisfactory.
Aug. 13	M	2	F	+ 5·9	− 3·5	− 1·8	

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				Arcturus.			
Mar. 30	M	2	F	− 3·4	−35·6	−30·8	Definition good.
May 7	M	2	F	+ 6·4	−54·1	−43·5	Spectrum tremulous.
21	M	2	F	+ 9·5	−49·0	−45·7	Star-line fairly well seen.
23	M	2	F	+ 9·9	−34·1	−34·2	Spectrum very bright.
28	M	2	b_1	+10·8	−69·4	−68·5	Star-line well defined.
June 1	M	2	F	+10·2	−60·3	−55·6	Spectrum bright.
4	M	2	b_1	+12·0	−47·1	−51·7	Spectrum bright & steady.
8	N	2	b_1	+12·6	−68·0	−57·1	Spectrum not very bright.
14	N	2	b_1	+13·4	−67·5	−65·2	
26	N	2	F	+14·6	−30·1	−30·6	Star-line distinct.
July 4	M	3	b_1	+15·0	−52·2	−54·7	Spectrum tremulous.
5	N	2	b_1	+15·1	−59·8	−57·9	
6	M	3	b_1	+15·1	−64·5	−64·4	Definition fair.
6	M	2	b_1	+15·1	−44·8	−47·4	Two-prism train. Definition poor.
9	M	2	b_1	+15·2	−54·1	−54·3	Spectrum tremulous.
13	M	2	F	+15·3	−54·6	−40·7	Spectrum tremulous.
17	N	2	F	+15·3	−58·0	−58·2	Spectrum very tremulous.
Aug. 13	M	2	F	+13·6	−46·6	−44·8	Spectrum tremulous.
21	N	2	F	+12·5	−46·0	−49·6	Star-line faint.
24	M	1	D_2	+12·0	−89·1	−79·9	Spectrum very tremulous and lines ill-defined.
24	M	1	D_1	+12·0	−44·0	−45·8	
				ϵ^2 Boötis.			
May 28	M	3	b_1	+ 8·9	−12·5	−14·9	Star spectrum very faint.
June 4	M	2	b_1	+10·0	−17·0	−20·3	Star-line very difficult to see.
July 5	N	2	b_1	+13·1	−53 6	−50·9	Definition very bad.
				β Libræ.			
May 24	M	2	F	+ 4·8	−33·4	−31·3	Star-spectrum & line faint.
31	N	2	F	+ 6·7	−25·9	−27·7	Spectrum faint.
				α Coronæ Borealis.			
May 16	M	2	F	+ 3·4	+28·3	+33·1	Spectrum steady. Definition fair.
23	M	2	F	+ 4·8	+12·4	+15·5	Spectrum steady and fairly bright.
Aug. 13	M	2	F	+12·6	+18·9	+18·6	Spectrum very tremulous.

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Serpentis.			
July 6	M	2	b_1	+13·0	+27·5	+32·8	Star-line faint.
				β Herculis.			
July 6	M	2	b_1	+ 9·2	−45·8	−50·3	Star-line faint & difficult.
				ζ Herculis.			
July 6	M	2	b_1	+ 7·4	−25·2	−25·0	Star-line difficult.
				α Herculis.			
May 28	M	2	b_1	− 1·8	−38·0	−43·6	Measures unsatisfactory.
				α Ophiuchi.			
May 23	M	2	F	− 4·5	+17·6	+17·1	Spectrum bright & steady.
24	M	2	F	− 4·3	− 9·7	− 6·9	Spectrum faint.
				γ Draconis.			
July 4	M	2	b_1	+ 1·3	+14·3	+14·5	Definition fair.
				α Lyræ.			
May 16	M	2	F	− 6·3	−43·8	−42·3	Definition very good.
24	M	2	F	− 5·5	−38·5	−37·1	Definition poor.
31	N	2	F	− 4·7	−18·1	−16·3	
June 26	N	2	F	− 1·3	−22·1	−22·7	
July 19	N	2	F	+ 1·9	−36·4	−42·0	
26	N	2	F	+ 2·9	−24·6	−27·5	
Aug. 3	M	2	F	+ 3·9	−37·8	−25·8	Spectrum faint.
14	N	2	F	+ 5·2	−51·5	−51·7	Spectrum tremulous.
23	N	2	F	+ 6·2	−37·5	−33·4	
Sept. 18	N	2	F	+ 8·2	−23·9	−23·1	
20	N	3	F	+ 8·3	−48·3	−45·2	Spectrum very steady.
Nov. 10	M	2	F	+ 7·3	−34·1	−36·4	Spectrum faint.
Dec. 5	N	1	F	+ 4·5	−23·9	−20·8	Spectrum very faint & tremulous.
				ζ Aquilæ.			
Aug. 14	N	2	F	+ 8·0	−26·9	−19·6	Spectrum faint & tremulous.
				δ Cygni.			
Nov. 16	M	4	F	+ 8·0	−17·3	−17·9	Spectrum faint & tremulous.

Date. 1883.	Obs.	No. of Meas.	Line.	Earth's Motion in M. per sec.	Concluded Motion of Star. Meas.	Estimd.	Remarks.
				α Aquilæ.			
July 19	N	3	F	− 0·8	+21·3	+20·0	
26	N	2	F	+ 1·0	− 6·5	+ 4·9	Star-line faint.
Aug. 3	M	2	F	+ 3·0	+15·3	+13·4	Spectrum faint.
23	N	2	F	+ 7·9	−23·0	−22·4	Spectrum bright.
Sept. 18	N	2	F	+13·1	−17·4	−13·1	Spectrum bright.
Oct. 13	M	2	F	+15·9	+ 5·7	+ 4·7	Spectrum faint.
22	M	3	F	+16·2	− 2·6	− 2·9	
Nov. 16	M	3	F	+15·0	− 5·0	− 5·1	Spectrum very tremulous.
				γ Cygni.			
July 4	M	2	b_1	− 6·3	− 4·2	− 4·2	Definition poor.
Nov. 10	M	2	F	+10·1	− 5·3	− 6·0	Spectrum very faint.
				α Cygni.			
June 14	N	3	b_1	− 8·4	+28·8	+27·1	Spectrum bright.
July 4	M	2	b_1	− 6·9	−27·5	−32·5	Star-line faint.
5	N	3	b_1	− 6·8	−41·9	−41·9	Definition poor.
Oct. 22	M	1	F	+ 7·7	−41·4	−39·1	Spectrum faint.
Nov. 10	M	2	F	+ 9·1	−56·4	−50·4	Spectrum bright.
				ε Cygni.			
July 5	N	2	b_1	− 7·8	−59·8	−60·2	Spectrum bright.
Nov. 10	M	2	F	+12·1	+ 9·5	+ 8·6	Spectrum faint.
				α Cephei.			
Nov. 16	M	2	F	+ 4·6	−39·1	−44·0	Spectrum tremulous.
				ε Pegasi.			
Nov. 10	M	2	F	+17·0	+16·4	+13·3	Star-line very faint.
				Fomalhaut.			
Sept. 20	N	1	F	+ 7·3	−43·4	−46·7	Spectrum faint.
				α Pegasi.			
July 26	N	3	F	−12·5	+13·0	+12·5	Star-line faint.
Aug. 16	N	2	F	− 8·0	+ 5·6	+ 2·6	Spectrum faint.
23	N	2	F	− 6·2	+ 1·9	+ 6·2	Star-line faint.
Oct. 13	M	2	F	+ 8·3	−39·0	−39·7	Spectrum very faint.
17	M	2	F	+ 9·4	−21·6	−20·7	Measures very satisfactory.

Date.	Obs.	No. of Meas.	Line.	Motion Measured.
		Sky.		
1883, June 6	M	5	b_1	− 3·4
July 4	M	5	b_1	+ 3·8
7	M	5	b_1	− 6·3
Aug. 25	M	4	D	0·0
Nov. 7	N	3	F	+ 1·1
27	M	5	b_1	− 1·7
		Moon.		
Jan. 15	M	3	F	+ 3·4
Feb. 16	M	5	F	− 0·7
23	M	5	F	− 1·0
May 15	N	4	F	− 3·0
16	M	5	F	+ 0·6
21	M	5	F	− 0·8
22	N	4	F	− 1·7
23	M	5	F	− 0·7
24	M	5	F	+ 1·0
June 14	N	2	b_1	− 1·1
26	N	2	F	− 2·2
July 13	M	5	F	+ 0·2
17	N	2	F	− 5·9
19	N	4	F	− 6·1
26	N	2	F	− 1·3
Aug. 13	M	3	F	+ 2·1
14	N	2	F	− 1·2
16	N	2	F	− 3·0
21	N	4	F	− 3·5
23	N	2	F	− 2·9
Sept. 18	N	2	F	− 2·5
20	N	2	F	+ 0·9
Oct. 13	M	3	F	+ 0·7
17	M	5	F	− 2·3
Nov. 10	M	5	F	− 0·5
12	M	5	F	− 0·8
16	M	5	F	+ 2·1
20	N	3	b_1	− 4·9

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1883.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs.		Phenomenon.	Telesc.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Obs.
1883.						h m s	
Jan. 20	(*a*)	Disapp. 71 Orionis	Altaz.	100	Dark	9 21 55·75	H.
Feb. 16	(*b*)	Disapp. χ^2 Orionis	„	100	„	5 59 4·82	L.
16	(*c*)	Disapp. χ^2 Orionis	E. Eq.	140	„	5 59 7·15	H.
Mar. 12	(*d*)	Disapp. *o* Arietis	„	60	„	6 51 3·74	L.
12		Disapp. *o* Arietis	Altaz.	100	„	6 51 3·51	H.
July 17		Disapp. Piazzi xvii. 304	„	100	„	11 30 31 99	H.
Aug. 21	(*e*)	Reapp. ϵ Piscium	E. Eq.	—	„	12 43 2·27	T.
24		Reapp. Piazzi iii. 87	Altaz.	100	„	11 21 29 40	A.P.
Sept. 14	(*f*)	Disapp. c^1 Capricorni	„	100	„	8 43 42·82	J.P.
14		Disapp. c^2 Capricorni	„	100	„	8 47 38·48	J.P.
14	(*g*)	Reapp. c^1 Capricorni	„	100	Bright	9 7 0·90	J.P.

Notes.

(*a*) Observation difficult; star disappeared very near the bright limb.
(*b*) „ cloudy, star very faint.
(*c*) Misty; star very faint. (*d*) Observation considered very good.
(*e*) Cloudy. (*f*) Star followed to bright edge; observation difficult.
(*g*) Moon's limb very tremulous.

Phenomena of Jupiter's Satellites.

Day of Observ.		Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ.	Mean Solar Time of N.A.	Obs.
1883.						h m s	h m s	
Jan. 4		I.	Ecl. R. First seen	E. Eq.	140	10 24 50	10 24 47	H.
			Full brightness	„	„	10 26 51		
		III.	First seen	„	„	11 42 51	11 45 8	„
			Full brightness	„	„	11 45 58		
	10	(*a*) II.	Tr. Egr. Bisection	„	„	7 58 9	7 56 0	„
	18	I.	Occ. D. First contact	„	„	11 10 35		
			Bisection	„	„	11 13 24	11 16 0	„
			Last contact	„	„	11 16 30		
		(*b*) III.	Occ. D. First contact	„	„	13 55 43	14 3 0	„
			Last contact	„	„	14 0 15		

Day of Observ.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ.	Mean Solar Time of N.A.	Obs.
1883.					h m s	h m s	
Jan. 18	I.	Ecl. R. First seen	E. Eq.	140	14 15 24	14 15 18	H.
		Full brightness	,,	,,	14 17 4		
27	I.	Ecl. R. First seen	Altaz.	100	10 40 28	10 39 33	B.
Feb. 5	III.	Tr. Ing. First contact	E. Eq.	310	10 51 31		
		Bisection	,,	,,	10 57 0	10 58 0	A.D.
		Last contact	,,	,,	11 4 59		
9	III.	Ecl. R. First seen	Altaz.	100	7 50 22	7 52 23	B.
10	I.	Occ. D. First contact	E. Eq.	140	11 3 20	11 9 0	H.
		Last contact	,,	,,	11 7 53		
16	III.	Occ. R. Bisection	,,	,,	7 0 0	7 3 0	,,
		Last contact	,,	,,	7 3 12		
		Ecl. D. Last contact	,,	100	9 15 30	9 13 20	,,
	(*e*)	Ecl. R. First seen	Altaz.	100	11 52 15	11 54 13	L.
		First seen	E. Eq.	140	11 52 18		H.
	II.	Occ. D. First contact	Altaz.	100	11 53 20		L.
		Last contact	,,	,,	11 57 35		,,
		First contact	E. Eq.	140	11 53 31	11 58 0	H.
		Bisection	,,	,,	11 55 58		,,
		Last contact	,,	,,	11 59 53		,,
23	III.	Occ. D. First contact	,,	310	7 58 0		
		Bisection	,,	,,	8 3 19	8 8 0	A.D.
		Last contact	,,	,,	8 8 40		
	III.	Occ. R. First seen	,,	,,	10 45 8	10 52 0	,,
		Last contact	,,	,,	10 50 52		
	III.	Ecl. D. Last seen	,,	,,	13 16 13	13 14 14	T.
26	I.	Occ. D. First contact	,,	,,	9 19 40	9 21 0	L.
		Last contact	,,	,,	9 21 57		
28	I.	Ecl. R. First seen	,,	60	7 20 48	7 20 9	A.P.
Mar. 6	I.	Tr. Ing. Last contact	E. Eq.	140	8 31 58	8 30 0	H.
	(*d*)	Tr. Egr. First contact	,,	,,	10 44 49	10 46 0	,,
		Bisection	,,	,,	10 46 25		
	(*e*) II.	Ecl. R. First seen	,,	100	11 32 24	11 31 24	,,
30	I.	Ecl. R. First seen	,,	310	9 32 17	9 32 20	L.
		Full brightness	,,	,,	9 33 20		
31	III.	Ecl. D. Last seen	,,	,,	9 15 49	9 16 41	A.P.
Apr. 6 (*f*)	I.	Occ. D. First contact	Altaz.	100	7 56 56	8 0 0	A.D.
		Last contact	,,	,,	8 0 10		

Day of Observ. 1883.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observ. h m s	Mean Solar Time of N.A. h m s	Obs.
Apr. 25	III.	Tr. Egr. First contact	E. Eq.	140	9 49 29	9 56 0	H.
		Last contact	,,	,,	10 0 27		
May 13 (*g*)	III.	Occ. R. First contact	Altaz.	100	8 47 22		L.
		Last contact	,,	,,	8 54 41		,,
		First seen	E. Eq.	140	8 48 12	8 52 0	H.
(*h*)		Bisection	,,	,,	8 50 51		,,
		Last contact	,,	,,	8 54 17		,,
16 (*i*)	II.	Occ. D. First contact	,,	,,	8 56 33	9 7 0	A.D.
		Last contact	,,	,,	9 2 32		
Oct. 16 (*k*)	II.	Occ. R. First seen	,,	310	12 12 41		
		Bisection	,,	,,	12 15 46	12 4 0	L.
		Last contact	,,	,,	12 19 15		
Nov. 6	I.	Ecl. D. Last seen	,,	60	12 49 8	12 49 7	J.P.
	II.	Ecl. D. Last seen	,,	,,	14 25 26	14 26 6	,,
13	I.	Ecl. D. First contact	,,	140	14 41 51	14 42 12	H.
		Last seen	,,	,,	14 42 58		
17 (*l*)	II.	Occ. R. Bisection	,,	,,	11 42 43	11 39 0	,,
		Last contact	,,	,,	11 46 10		
19	III.	Tr. Ing. First contact	Simms Eq.	—	11 16 54		
		Bisection	,,	—	11 19 57	11 24 0	,,
		Last contact	,,	—	11 24 22		
23	I.	Tr. Egr. Clear of Planet	E. Eq.	—	11 46 37	11 47 0	T.
Dec. 1 (*m*)	I.	Occ. R. First seen	,,	60	10 45 15	10 45 0	L.
		Full brightness	,,	,,	10 47 30		
14	III.	Ecl. D. First contact	,,	70	8 33 2	8 36 24	H.
		Last seen	,,	,,	8 37 57		

Notes.

(*a*) Cloudy at first and last contact. (*b*) *Jupiter's* limb ill-defined, boiling. (*c*) Observation doubtful: perhaps a few seconds late. (*d*), (*e*) Clouds passing. (*f*) *Jupiter* very tremulous. *g*) First contact doubtful; last considered very good. (*h*) Observation doubtful. (*i*) Observation uncertain; planet low down; *Jupiter's* limb boiling. (*k*) *Jupiter's* limb very badly defined. (*l*) Observation uncertain; *Jupiter's* limb boiling; clouds passing. (*m*) *Jupiter* very faint; sky hazy.

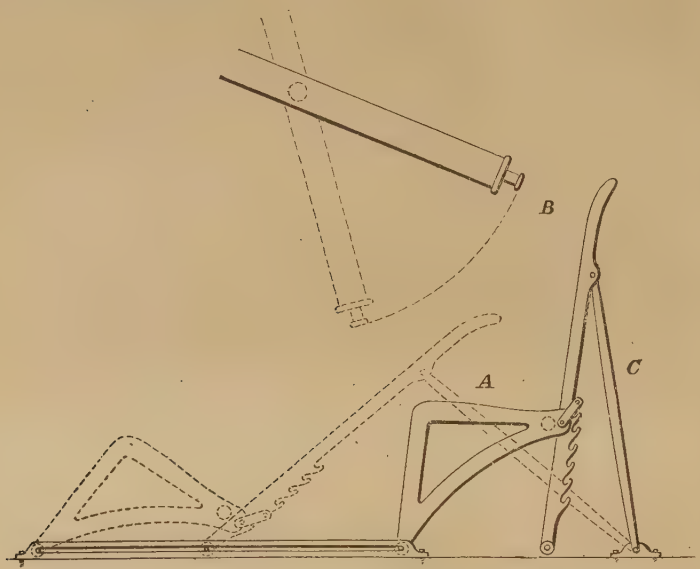
The clear aperture of the object-glass of the E. Equatorial is $6\frac{3}{4}$ inches, of the Altazimuth 4 inches, and of the Simms Equatorial 6 inches.

The initials A. D., T., L., H., J. P., A. P., and B., are those of Mr. Downing, Mr. Thackeray, Mr. Lewis, Mr. Hollis, Mr. Power, Mr. Pead, and Mr. Bennett.

Royal Observatory, Greenwich:
1884, *Jan.* 10.

Suggestion for an Observing Seat for Transit Instruments.
By Richard Inwards.

The accompanying drawing shows the proposed form of seat. It consists mainly of three moveable parts—the seat A, the back B, and the strut C, the latter being attached to a hinge-piece screwed into the floor.



The seat is raised or depressed by altering the position of the supporting link upon the rack of B. This at the same time alters the height of the seat and the slope of the back, as well as thrusts the whole chair forward by the action of the hinged strut C, as shown by the dotted lines in the diagram. This enables the observer to follow with comfort all the usual movements of the telescope. The addition of a small moveable pad or cushion behind the head may be necessary to complete the adjustment in some positions. A roller keeps the seat and back always in contact.

A seat on a similar principle can be made in which the observer can by means of a tangent screw and rack alter his position without leaving the seat. A model illustrating this was exhibited to the meeting.

1884, *Jan.* 11.

On the Latitude of the Observatory, Windsor, N.S. Wales.

By John Tebbutt.

The latitude hitherto adopted (namely, $-33^\circ\ 36'\ 28''\cdot9$) is that of the prime vertical pier of the old Observatory as deduced from prime vertical observations with a 2-in. Transit instrument during the years 1867, 1868, 1869, 1870, and 1874. The stars employed were nine in number, culminating within $39'$ of the zenith. Their positions, however, rested almost wholly on observations with the old Parramatta Transit Circle at the Sydney Observatory, and were therefore very imperfect. Of the ninety-three observations fifty-nine were made on the middle wire only of the Transit instrument, and thirty-four on a side wire with the instrument reversed, according to Struve's method, both before and after the meridian passage. I have recently investigated a correction to this latitude from more accurate places of the stars as obtained from the Sydney Catalogues for 1877 and 1878, the Melbourne Catalogue for 1870, the Cape Catalogue for 1880, and recent MS. observations kindly communicated by Messrs. Ellery and Russell. The new result is $-33^\circ\ 36'\ 29''\cdot1$, and the reduction $-0''\cdot4$ applied to this value gives $-33^\circ\ 36'\ 29''\cdot5$ as the latitude of the piers of the Meridian Transit and Equatorial instruments in the new Observatory adjoining, erected in 1879. During the year 1881 I made a new investigation of the latitude by means of an excellent 3-in. Transit instrument temporarily mounted on the prime vertical pier of the new Observatory, the local times being obtained for the purpose by means of the old 2-in. Transit. The stars selected for observation were Nos. 3908, 3916, and 4180 of the Washington Catalogue for 1860, and Nos. 4015, 4309, and 879 of the B.A. Catalogue for 1850. The mean places were adopted from the Southern Catalogues above-mentioned and MS. results obtained by Messrs. Ellery and Russell in 1881, and were weighted according to the number of observations from which each was derived. Proper motion was applied from the best authorities available. The transit system of the 3-in. telescope consists of five threads, and so far as the first five stars are concerned the method adopted was to observe the transits east and west across as many threads as possible, the rotation axis of the instrument being in the same position throughout the observations of each evening. The pivots were, however, reversed between the observations of different evenings. In the case of the last star the transits were observed four times each evening across one or more of the side threads according to Struve's method described in Chauvenet's "Spherical and Practical Astronomy" (1874, vol. ii., p. 262). A value of the latitude was then deduced from each pair of observations of a thread with the pivots in the same direction, and it was thus assumed that the collimation error, which was itself very small throughout the series of observations, would be eliminated from the mean result. During the observations of B.A.C. 879 the opportunity was also taken to note the transit across the middle

Date of Observations.	Star.	Mean Place of Star 1881·0. R.A.	Declin.	Authorities.	Number of Separate Results. Clamp N.	S.	Concluded Seconds of Latitude.
1881.		h m s	° ′ ″				″
Apr. 4, 6, 7, 8, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 3908	9 13 47·83	−33 35 42·3	Sydney, MS. Obs. 1881, Melb. MS. Obs. 1881, & Cape Cat. 1880	14	14	30·8
Apr. 4, 6, 7, 8, 9, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 3916	9 14 51·87	−33 36 0·9	Sydney MS. Obs. 1881, Melb. MS. Obs. 1881, & Cape Cat. 1880	15	15	31·1
Apr. 7, 8, 11, 12, 13, 14, 15, 16, 19, 20	W.C. 4180	9 56 48·87	−33 36 4·4	Melb. MS. Obs. 1881, & Cape Cat. 1880	10	14	30·8
May 4, 6, 9, 10, 11, 12, 13, 14	B.A.C. 4015	11 46 53·98	−33 14 45·6	Melb. Cat. 1870 Cape Cat. 1880	18	16	31·0
May 9, 10, 11, 12, 13, 14	B.A.C. 4309	12 44 13·95	−33 21 0·2	Sydney Cat. 1877 Melb. Cat. 1870 Cape Cat. 1880	14	14	31·0
Dec. 10, 17, 21, 23	B.A.C. 879	2 44 6·65	−32 54 22·4	Sydney Cat. 1878 Cape Cat. 1880	8	8	30·6

thread in opposite positions of the rotation axis as described on pages 263 and 264 of the work above cited. I may remark that the equatorial distances of the side threads from the middle thread had been very carefully determined, and that the level employed was a sensitive one of about 1130 feet radius, each division being equal to 0″·8. The individual results throughout the series were highly satisfactory. The table on p. 102 exhibits an abstract of the work on each star.

Three observations of B.A.C. 879 on the middle thread as previously described gave a result of 30″·5. The mean of all the determinations gives −33° 36′ 30″·9 as the latitude of the instrument, to which if we apply the reduction +0″·1 we get −33° 36′ 30″·8 as the latitude of the piers of the Meridian Transit and Equatorial instruments of the new Observatory. This value, which differs 1″·3 from that obtained by means of the old Transit and an imperfect level, will in future be adopted as the geographical latitude of the Observatory.

Observatory, Windsor, N.S. Wales:
1883, *Nov.* 27.

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*Note on M. Loewy's Communication referring to an Apparatus for the Determination of Flexures. With some Remarks on Professor Houzeau's (and Mr. Schaeberle's) Proposal on the same Subject.** By A. Marth.

Before submitting to the critical consideration of readers some remarks on M. Loewy's "Réponse," &c., *Monthly Notices*, XLII. p. 287 ff., I may perhaps be allowed to quote the words in which M. Loewy expressed his opinion on my old proposal in his paper, "Nouvelle Méthode pour Déterminer la Flexure des Lunettes," published in the *Comptes Rendus* of December 9, 1878, vol. lxxxvii. p. 891, so that readers may form their own judgment upon the objections, which, as I stated at the last December (1881) meeting, have been such a puzzle to me.

"M. Marth, astronome anglais, a proposé d'établir, dans le cube central de la lunette, et à peu près sur l'axe de rotation, un

* The present paper (which is that alluded to on p. 349 of vol. xlii.) was intended to be published in the Supplementary Number of vol. xlii. of the *Monthly Notices*. But when I learnt that M. Loewy had already sent a second "Réponse," I thought it better to postpone the publication of my own paper, in order to give him precedence, and to avoid cross-writing. Owing to temporary absence in South Africa, I did not get a sight of M. Loewy's second paper till the end of December, when I saw the Supplementary Number at the Cape Observatory. That second "Réponse" made me the more desirous that interested readers, for whose critical examination I had asked already (*vide* p. 349 of vol. xlii.), might form their own judgment before they read what I may have to say, and the delay will have given them time to do so. Under the circumstances I have thought it better to make some alterations, and to indicate my own opinion rather more plainly than I had first intended.

appareil se composant de deux objectifs, dont chacun a la moitié de la distance focale de l'instrument, et entre lesquels se trouve placé un miroir argenté percé à son centre.

"Théoriquement, on pourrait arriver à évaluer le déplacement de l'objectif par rapport à un point du réticule, mais il faudrait, pour cela, recourir à des hypothèses qui, dans la réalité, ne peuvent être nullement confirmées. Il faut supposer que le miroir et les deux objectifs se trouvent, dans la rotation de la lunette, reliés ensemble d'une manière invariable, condition presque impossible à réaliser ; il faut faire abstraction de l'effet de torsion que produit sur l'appareil la flexion du cube central auquel il est attaché et tenir compte de l'effet de sa propre pesanteur. On voit immédiatement que l'appareil auxiliaire lui-même peut introduire des erreurs de même ordre ou des inexactitudes encore plus grandes que celles que l'on cherche à évaluer."

This latter assertion is directly at variance with the opinion which I had expressed on the same point in § 1 of my old paper (*Astron. Nachr.* vol. lvii. p. 260), in case the construction is adapted to its purpose (*bei zweckmässiger Einrichtung*), so that there must be some misunderstanding or some flaw of reasoning on the one side or the other. Where was the flaw? On the one hand, I gave credit to M. Loewy that an astronomer in his position would not make assertions without due consideration, so that my not perceiving, even after searching, what M. Loewy sees "immédiatement" might be due to some fault on my part. On the other hand, the very word "immédiatement" suggested a doubt, and this doubt was not lessened when, reading on, I found that his new method, so far as it refers to the flexure of the telescope, was merely one of the variations of my own proposal, which I had duly considered, and duly rejected, some years before the publication of my old paper. However, as M. Loewy's objections were for me rather enigmatical, I had to wait till further information might throw some light upon them; and I had to content myself meanwhile with occasional (unsuccessful) enquiries of astronomers, who were going, or had been, to the Paris Observatory, whether they could enlighten me, what meaning was to be attached to the words "effet de torsion que produit sur l'appareil la flexure du cube central," and how M. Loewy's own apparatus was fastened. In November last (1881) I got an opportunity of seeing the memoir of MM. Loewy et Périgaud, "Etude des flexions du grand cercle méridien . . . et de la forme des tourillons à l'aide de l'appareil imaginé par M. Loewy," extracted from vol. xvi. of the *Annales de l'Observatoire de Paris*; and when the subject of flexure came up incidentally at the December meeting, I made the statements referring to my old proposal and its first embodiment in Messrs. Cooke's apparatus, to M. Loewy's objections and to his description of his own apparatus, &c., which are alluded to on page 345. I expressed also my hope that, when Messrs. Cooke's apparatus should be exhibited at one of the

meetings, those who took an interest in the matter would exercise their critical faculties upon it and compare it with M. Loewy's, described in his memoir.

M. Loewy has not waited for a little information about the actual instrument, but has, in his "Réponse," &c., already made a comparison, so that I have at last become enlightened about what his objections really are.

Strange to say, of all the "nombreux mouvements indépendants les uns des autres," which, according to M. Loewy, the lenses and the mirror of the apparatus undergo during the rotation of the instrument, he singles out the one: "que le flint et le crown peuvent se déplacer l'un par rapport à l'autre. . . ." Of course, if we assume great carelessness and negligence on the part of the observer as well as of the instrument-maker, that may be possible. But are there readers who object seriously to the employment of achromatic object-glasses for that reason? On the last page of his memoir, M. Loewy gives some account of an experiment (to which I shall have to refer further on) to ascertain whether the two lenses of the object-glass of the Bischoffsheim circle glide upon one another during the rotation of the telescope, and he draws the conclusion, "les deux disques constituent donc un bloc invariable," so that, according to his own showing, the fault in question is avoidable, even in the case of an object-glass of considerable aperture. But in the case of small object-glasses, such as those employed in my apparatus, the fault is not only far more easily guarded against, but, if it were needed, there is available a simple remedy—the cementing of the lenses. In reference to the mounting of his own lens, M. Loewy says (on page 6 of the "Etude," &c.): "Pour rendre absolument solidaires le verre et sa monture et éliminer tout mouvement relatif de ces deux appareils, on les a collés ensemble à l'arcanson." *Absolument* solidaires! If the panacea is to be considered so very efficacious in the one case, why should it lose its efficacy in the other? What becomes then of this, the only one of M. Loewy's objections which is not quite vague? But, it so happens, that in my apparatus it would not be really necessary that the two lenses, which form each of the two small object-glasses, should be "absolument solidaires." For they might be allowed to glide, and, provided the amount of the gliding is a regular periodical function of the direction of the telescope, its effect upon the results would be got rid of, in case the conditions laid down in § 3 of my old paper are complied with. Does it require more than a little reflection to see this?

In order to appreciate at their true value M. Loewy's other objections, let readers first examine the reasons which induced me some twenty years ago to reject that variation of my proposal, which M. Loewy treats as if it were a new method.

Astronomers who take an interest in the flexure problem, and who have ever read the "Description of the Transit Circle" in the Appendix of the *Greenwich Observations* of 1852 (reprinted in

the volume of 1867) are acquainted with the experiments, which were made at Greenwich in September 1850, in order "to determine the absolute flexure of the telescope tube as attached to the axis." The Astronomer Royal's account of these experiments is found on page 17 of the description, and is as follows:—

"Before mounting the 12-ft. object-glass, an object-glass of about 3-ft. focal length was mounted in the centre of the cube, and a cross of wires was fixed at the object end, so that its image was formed upon the wires of the eye-piece. A better image would have been formed if two 6-ft. object-glasses had been used with their external surfaces opposed to each other. The image, however, was quite good enough for this determination. The place of the image at the eye end was measured with the declination micrometer when the telescope was horizontal with the object end north, and again when the object end was south. The difference of the two readings found on 1850 September 2 and September 14 (the two results being in precise accordance), was $0^{r}.204 = 0.00414$ in., one half of which, or 0·00207 in., was the effect of flexure in lowering one end with respect to the line passing through the centre and the other end. If the two ends yielded equally, the drop of each was sensibly $\frac{1}{1000}$ part of an inch.

"This determination, it will be remarked, is one of curiosity only."

So far the statement in the Greenwich volume. Years ago I have made some inquiries about the manner in which the cross of wires was really fixed, but have not succeeded in getting the desired information, so that I am not sure whether the inference which has been drawn from these experiments may not require some modification. However that may be, these experiments indicated a way to determine the *sum* of the flexures of the two ends of a telescope; and though this determination was avowedly one of curiosity only, and of no direct astronomical interest, it set me thinking whether the determination of the *difference* of the flexures of the ends of a telescope, and therewith that of the astronomical flexure, might not be accomplished by a suitable arrangement. When I had settled in my mind that this could be done by substituting for the cross of wires a mark or marks near the optical centre of the object-glass and by arranging the apparatus so that it should show in the plane of the webs or lines at the eye end not only an image of the mark, but also a reflected image of the webs themselves, and when I had thus found the simple theoretical solution of the problem, I had to consider and compare the advantages and disadvantages of various schemes for making this solution available in practice, and to select that which promised to be the most efficient.

In order that the apparatus may truly answer its purpose, it must be fastened to the instrument mechanically in such a way that its rotation is exactly determined by that of the divided circle, and it must be so constructed that the results can be

freed from all systematic errors. The determination or elimination of any periodical errors represented by $\alpha \sin z + \beta \cos z$, and by similar terms depending on $3z$, $5z$, &c., can be directly effected by making the apparatus reversible, and this advantage can be secured in case the object-glass and the eye-end webs of the instrument are made equidistant from the axis of rotation. The optical part of the apparatus must, of course, be so arranged as to give sharp images of the mark and of the webs simultaneously. Which scheme promised then to be practically the best?

Taking first the case of a single object-glass, as in the Greenwich experiment, or the still simpler one of a single lens, the reflected image of the webs could obviously be procured by converting a portion of the surface of the lens into a concave mirror with a radius of curvature equal to the distance from the webs; but the question was whether the central or the outer part of the surface should be made the mirror. If the central portion was used for the purpose, the lens could be made symmetrical and reversible, as there would probably be no practical difficulty in converting the middle part of a double convex lens into a double concave one. But what sort of image of the mark would be got in that way? If the outer portion of the lens was formed into a mirror, there would be the practical difficulty of making the central portion of a double concave lens double convex. Of course, by abandoning the condition of reversibility and symmetry, the lens might be made a meniscus. But, in that case, how could the error of rotation of the mirror's axis be determined or eliminated? And how could sharp focusing be effected simultaneously for both images? The condition of reversibility may be maintained by leaving the middle part of the double concave mirror perforated, and by inserting in the perforation a double convex lens of proper focal length. By making the mirror lens thick enough the great advantage would be gained that the sides opposite to the pivots could themselves be converted into such plane mirrors as I require for the determination of the flexure of the axis and of the pivot errors. In order to get a better image and to allow exact focusing, two crossed lenses or two small achromatic object-glasses may then be inserted, instead of the single lens, in the perforation. The whole apparatus might indeed be made of glass, so that the mirrors and the end-pieces, which are fastened to the instrument, are formed out of one body. But though the making of such a body of glass might not be difficult, it could probably only be procured by special favour of the glassmaker, and the converting of it into the intended apparatus would also require special willingness on the part of the optician, so that the conditions of its construction would be rather exceptional.

Refraining then from assuming more than the ordinary resources of the optician's workshop, I had to consider the case in which the optical part of the flexure apparatus is composed of two lenses or two achromatic object-glasses and of a plane mirror,

covering about one-half of their apertures, between them. Of the motions of rotation and translation to which the various parts of the apparatus are liable, two, and only two can affect the determination of the astronomical flexure, namely the error of rotation of the mirror, and the difference of translation of the optical centres of the two lenses or of the two object-glasses. Let these optical centres, p and q, the mark o on the great object-glass, and the assumed zero-point r of the eye-end micrometer be referred to a system of rectangular co-ordinates, one axis of which is the actual axis of rotation, while the other two axes rotate with the circle, the x axis being approximately in the direction of the axis of the telescope. In order to define the position of the mirror, let ρ denote that point in the plane of the micrometer webs which is normal to the plane of the mirror at the point where the x axis cuts it. As the effects of flexure in the two directions of the circle- and of the transit-observations are investigated separately, though in the same way, the ordinates of the five points $o\ p\ q\ r\ \rho$ in the two directions may be conveniently denoted by the same letters and as the points themselves, so that in the one case $o\ p\ q\ r\ \rho$ signify the distances of the five points from the plane passing through the x axis and the axis of rotation, and in the other case their distances from the plane passing through the x axis perpendicular to the axis of rotation. The micrometer readings O and R (in the notation of my old paper) at which the moveable webs coincide with the image of the mark and with their own reflected image represent the ordinates of these two points in reference to the zero-point of the micrometer. Their ordinates in the assumed system are therefore $r-O$ and $r-R$, in case the micrometer measures are reckoned positive in the opposite direction to that of the ordinates. As the line from the mark o to the optical centre p is parallel to the line from q to the image of the mark, we have, if the two lines are of unequal length in the proportion of $\kappa : 1$,

$$\frac{o-p}{\kappa} = q-(r-O) \quad \text{or} \quad o+\kappa r-p-\kappa q=\kappa O.$$

Further

$$r-R=\rho+q \qquad \text{or} \qquad r-\rho-q=R.$$

Hence the equation for the correction $o-r$ of the astronomical flexure

$$o-r+(1+\kappa)\rho-p+q=\kappa O-(1+\kappa)R.$$

which shows that, in addition to the error ρ of the rotation of the mirror, the difference $p-q$ in the motion of the two optical centres may affect the determination of flexure, and that it must be freed from the influence of these two sources of error. By making the apparatus reversible in reference to the axis of the telescope and accordingly symmetrical and equidistant from the mark

and from its image, the equations become, since in that case $\kappa=1$,

$$o+r-p-q=0$$
$$r-\rho-q=R$$

and therefore, if the flexure correction $o-r$, as a function of the zenith-distance or of the circle-reading z, is now called fz,

$$fz+2\rho-p+q=0-2R.$$

Assuming that o and $p-q$ are likewise functions of z, their periodical terms of the form $\alpha \sin z+\beta \cos z$, and similar terms depending on the uneven multiples of z, have opposite signs in the reversed position of the apparatus, the mean of the observations in the two positions becomes therefore free from these terms, and the remaining question is merely whether the apparatus is constructed and mounted with sufficient care so as to prevent the existence of other terms and terms which are not functions of z (as, for instance, the shaking of the mirror and lenses).

If the optical part of the apparatus could be constructed effectively out of one piece of glass, there would be only one optical centre instead of two, the ordinates p and q would be identical, and therefore

$$o+\kappa r-(1+\kappa)\, q=\kappa O, \text{ and}$$
$$fz+(1+\kappa)\,\rho=\kappa O-(1+\kappa)R,$$

the equation for R being the same as before. Provided that the O and R observations are made at the same time or while the telescope is undisturbed, so that the value of q remains the same for both, the flexure equation would then be affected only by the error ρ of the rotation of the mirror's axis. But is there any trustworthy method available for determining this error without making the apparatus reversible? Or, in making it reversible, is there any way of overcoming the optical difficulties which I have mentioned before?

It would lead me too far here to speak of other schemes for the practical application of the principle of my proposal, as the question at issue is narrowed to a comparison between the scheme which I adopted and the meniscus scheme. In considering the latter, I could not think of a trustworthy method for testing and determining the all-important error of rotation of the axis, nor get over the optical difficulty. In the other case I could plan the apparatus so as to provide for the determination or elimination of any plausible error which might affect the results, to secure, even under variable conditions, good images by arranging for exact focusing without disturbing the mirror, and to test and control the efficiency of the apparatus in several ways. Hence I had no difficulty and no hesitation in coming to a decision between the two schemes.

I will now turn again to M. Loewy's assertions and objections.

When, in reading his paper in the *Comptes Rendus* of Dec. 9, 1878, I found that what M. Loewy represented as a new method

was merely one of my old rejected acquaintances, I was, of course, curious how he managed to meet the difficulties on account of which I had discarded the meniscus scheme; but I had to wait till I saw the *Étude* &c., before I could learn what provisions he makes for getting good images and for exact focusing, and in what manner he solves the problem of determining the error of rotation of the mirror.

As regards the images, the mirror is first shifted in the direction of the optical axis till a satisfactory reflected image of the webs is seen. "La place du miroir étant ainsi obtenue, s'il arrive que les traits de l'objectif ne donnent pas dans le champ une image nette, on corrige l'une des deux surfaces [of the convex-concave lens] jusqu'à ce que la netteté soit obtenue" (v. *Etude*, page C. 7). Some information about the image of the mark may be gathered from the *Nota* on the last page (C. 120). "Pour vérifier si les deux verres de l'objectif ne glissaient pas l'un sur l'autre . . ., nous avons au milieu du disque intérieur fixé une petite goutte d'argenture . . ., sur laquelle nous avons tracé deux traits en croix, et nous avons mesuré . . . les distances de cette croisée de fils et d'un point de la surface extérieure de l'objectif, c'est-à-dire de la croisée de deux traits; la différence a éte constante . . . Les deux disques constituent donc un bloc invariable." The two crosses are separated by the thickness of the great object-glass, so that the conjugate foci, where their images are formed, are separated by a corresponding interval. But, of course, if the two images can be seen simultaneously with such distinctness as to allow exact measurements, why should M. Loewy trouble himself about exact focusing, or make provision for it? Though I have not had an opportunity of testing the optical part of Messrs. Cooke's apparatus, I need hardly say that their two object-glasses will certainly be found unfit to allow such a feat as M. Loewy's to be performed. In case the marks on the two lenses of the great object-glass are not to be put on their inner surfaces (and the reason to be taken into consideration is, that in that case the marks could not well be formed by cross-lines, but would have to be points or little circles), but on their outer surfaces, the contrivance for exact focusing must be resorted to, and the marks examined separately. It is to be noted that the diameter of the effective aperture of the convex-concave lens, which M. Loewy employs, varies (C. 3) for different telescopes from 8 to 15^{mm}, or is at most $0^{in.}6$.

And now, in what way does M. Loewy solve the problem of determining the error of rotation of the mirror's axis, the unknown ρ in the equation $fz+(1+\kappa)\rho=\kappa O-(1+\kappa)R$, on the elimination of which the determination of the flexure correction fz depends? On page C. 9 he states: "l'axe de suspension de notre appareil étant justement perpendiculaire à [l'axe est-ouest], ce mouvement angulaire [autour de l'axe est-ouest] se trouve rendu impossible par l'effet même de la construction. Du reste, la comparaison des résultats trouvés pour l'objectif et le réticule

a justifié cette assertion." That M. Loewy has satisfied himself on this point may be learnt from page C. 28, where he says: "après avoir démontré rigoureusement, par la concordance des valeurs de a [a denoting the motion of translation of the optical centre or the q of the equations in the present note], qu'il n'existe pas dans notre appareil de mouvement tournant autour de l'axe [est-ouest], ce qui était du reste à prévoir, comme nous l'avons dit, nous avons, pour plus de rigueur, repris l'ensemble des équations . . ., pour en tirer . . . les trois inconnues f_r, f_o and a" [the r, o and q of the equations before given].

It appears, therefore, from M. Loewy's assertions, first, that the error ρ, or the rotatory motion in question, is rendered *impossible* by the very construction of the apparatus; and, secondly, that he has in some way *rigorously demonstrated* that in his apparatus it does not exist.

For those readers who have not access to the *Étude* it may be necessary at least to indicate the manner of M. Loewy's demonstration. He first simplifies the equation for the observations of the reflected image $r-\rho-q=\mathrm{R}$, by simply omitting the unknown ρ altogether, so that the equation becomes $r-q=\mathrm{R}$, or, in his notation,

$$f_r - a = m.$$

The equation for the image of the mark is

$$f_r - a\left(1+\frac{\alpha}{\beta}\right)+f_o\frac{\alpha}{\beta}=n$$

[$\frac{\beta}{\alpha}$ and n being the κ and O of the former equations]. If the m and n observations are made at the same time, or while the telescope is left undisturbed, so that the a in the two equations have strictly the same value, the flexure correction f_o-f_r is quite unaffected by whatever the value of a may be, for, as the unknown ρ has been got rid of, the flexure correction becomes simply

$$f_o - f_r = \frac{\beta}{\alpha}n - \left(1+\frac{\beta}{\alpha}\right)m.$$

The dates of the m and n observations show that the observations have not been made at the same time, and consequently that the simple precaution to eliminate a entirely has not been taken; moreover, M. Loewy requires the value of a for his attempt to find the flexure of the axis. Otherwise, the knowledge of a would be a matter of indifference, as it is in the case of my apparatus, where neither the investigation of the flexure of the telescope nor that of the axis is affected by it. In order to determine the value of a, M. Loewy repeats the observations, after having attached extra weights outside the framework which holds the lens, and after having thus altered the whole weight of the apparatus. He then makes the hypothesis ("évi-

dente *à priori*," as he says, p. C. 17) that the value of a is in proportion to the whole weight, satisfies himself in the case of the great meridian circle that "on peut donc conclure l'absolue proportionalité des effets dus à la pesanteur de l'appareil" (C. 24), and in the case of the Bischoffsheim circle he simply assumes the correctness of the hypothesis. From the accordance of the values of a deduced from the m and from the n observations he concludes the non-existence of any rotatory motion, and he calls this conclusion a rigorous demonstration. But what hypothesis he makes in coming to this conclusion M. Loewy does not say. Though in the investigation of the side-flexure the same hypothesis is disproved by the observations (C. 62), M. Loewy is not disconcerted. "Cette particularité fâcheuse provient de ce que l'axe de notre appareil n'était pas fixé d'une manière assez solide dans les coussinets;" and he thinks l'arcanson would have prevented it.

After my first reading of the *Étude*, I had wondered that the apparatus should have been used in one position only. For, though it cannot be reversed in reference to the axis of the telescope, it can at least be inverted or reversed in reference to the axis of rotation. And though agreement of the results obtained from the observations in the two positions might not prove much, and could not be a test such as agreement after reversion in reference to the axis of the telescope would be, it would at least be worth something. When I made my remarks at the December meeting 1881, I little anticipated what curious revelations they would bring to light. From M. Loewy's *Réponse*, &c., on page 288, readers have now learnt that, after all, observations in the inverted position of the apparatus have been made, and that he is in possession of evidence of very decisive character. I must beg my readers to refer to the paper itself so that I need not quote the whole of the context. M. Loewy states there: "nous avons retourné l'appareil pour opérer dans des conditions différentes, et . . . nous avons constaté que le mouvement de l'instrument dans la position inverse était tout-à-fait différent du premier et qu'il était impossible d'établir aucune relation entre les deux." The conclusion which M. Loewy draws is found a dozen lines further down: "Il est donc évident que ce qui se présente pour un appareil aussi simple que le mien existe *à fortiori* pour la disposition beaucoup plus compliquée de M. Marth."

But what are the true conclusions which are to be drawn from his statements? If it has been found impossible to establish any relationship between the motions of the instrument in the two positions, then it is evident that there is something wrong about the apparatus. If M. Loewy's statement implies that this want of any relationship is established for the astronomical flexure, the source of the mischief is obvious, for in the meniscus scheme the only cause which can produce such an effect is the rotatory motion ρ. After M. Loewy's previous assertions

in the *Étude*, *first*, that the existence of this rotatory motion is rendered impossible by the construction of his apparatus, and, *secondly*, that he has rigorously demonstrated that it does not exist, his statements on page 288 lead, on the contrary, to the logical conclusion, *thirdly*, that this rotatory motion *does* exist in his apparatus, that hitherto the law which it follows has not been found, and that consequently the results obtained cannot be cleared from the influence of this rotatory motion.

The inference which M. Loewy himself draws is, of course, quite different. Unconscious of what the fact that his apparatus does not sustain even the test of mere inversion really proves, he deduces from it and from some other peculiarity of his apparatus the curious "Il est donc évident," &c., which I have quoted. Will readers consider a little the construction of the two instruments, the one carefully planned and constructed so as to form a compact body which can be inserted in its place and taken out and re-inserted in its inverted and reversed position with the greatest ease and confidence; the other constructed with such want of ordinary forethought and caution that it cannot be put in its place or taken out without being taken to pieces—and will they then form their own opinion?

M. Loewy's repeated assertions of the simplicity of his apparatus receive some rather curious and telling commentaries in the Paris Reports. In the *Rapport Annuel sur l'état de l'Observatoire de Paris pour l'année* 1880, on p. 3, Admiral Mouchez states: "M. Loewy s'occupe activement de l'étude du cercle du jardin . . .; pendant quatre mois, cet instrument a été distrait du service régulier pour l'étude de la flexion." And in the *Rapport pour* 1881, p. 4: "En tenant compte que, pendant cinq ou six mois, une partie du personnel a été employée jour et nuit à la délicate opération de la mesure de la flexion et des erreurs de division du cercle du jardin, et que, pendant cette même période, les observations astronomiques ont été nécessairement suspendues à cet instrument, on jugera que le personnel du service méridien a . . . rempli sa tâche."

I have probably given sufficient explanations to enable anyone to estimate the statements and assertions and objections in M. Loewy's *Réponse* one by one at their true value, and to judge for himself. M. Loewy sums up his objections in the following sentence: "En résumé, pour étudier la variation d'une ligne de visée unique, pendant la rotation de la lunette, M. Marth admet, contrairement à la réalité, l'invariabilité des cinq lignes de visées différentes provenant des cinq pièces optiques composant son instrument."

The phrase sounds very well, but is there any truth in M. Loewy's assertion? I assume the five pieces of glass to be in motion, and examine the effect of their motions according to the optical theory which I hold to be correct (v. p. 346), and according to which I have to do with the investigation of the motions of two points and of a plane. Since M. Loewy asserts

that I admit the invariability of five lines of vision, he must have some optical theory according to which there are five such lines "provenant des cinq pièces optiques" to be considered. What five lines of vision are meant? By what points are they defined, or through which points do they pass?

When I get enlightened about these five lines, there will perhaps be no difficulty in pointing out where the flaw in the reasoning on the one side or the other is to be found, and I will state the result of my examination in a Postscript. The settlement of this question will also settle whether what M. Loewy in his statement quoted at the beginning of this paper asserts is true or not.

For the present I will conclude with inviting my readers to make a little comparison of M. Loewy's method of determining the pivot-errors, not with my own method, but with the old well-known method of examining the pivot-errors by measuring the coordinates of dots at the ends of the pivots by means of microscope-micrometers fixedly mounted upon the piers. The method was first practically tried, if I am not mistaken, by Taylor at Madras half a century ago, though with inadequate means, and it has since been employed at Greenwich, at Cambridge, at Paris, and elsewhere, and, so far as I know, it has given satisfaction.

In his own method M. Loewy likewise determines the coordinates of a dot near the axis of the perforated pivot in reference to a fixed point; but there is this difference, that, while in the old method the fixed point is the zero point of the micrometer itself and the coordinates of the dot are directly referred to it, in M. Loewy's arrangement the fixed point, or what is intended to be the fixed point, is produced by a beam of light throwing a luminous image of the centre of a perforated disk upon the glass plate containing the dot, and the relative positions of the dot and of the luminous image are then measured by the eye-end micrometers, as their images are visible there by means of a part of the flexure-apparatus acting as a prism.

And then, M. Loewy's actual application of the altered method? For some reason, which is not explained, the investigation of the pivot-errors as well as that of the flexure-errors is confined to a number of directions of the telescope extending over only one-half of the rotation, and is not carried out for the other half. The results are consequently incomplete and deprived of that degree of trustworthiness which would have been attainable had the measurements been made in the same number of directions but distributed over the whole circumference. The investigation of the errors of the western pivot of the great meridian circle leads merely to the conclusion "... nous sommes en droit d'admettre que nous pouvons, sans qu'il en résulte d'erreurs sensibles pour les réductions, considérer le tourillon percé comme cylindrique ..." M. Loewy then proceeds (*Étude*, p. C. 93): "Il eût été, sans nul doute, très intéressant de

faire la même étude de la forme du tourillon *est*; mais, ce tourillon n'étant pas percé, il ne nous était pas possible d'appliquer nos procédés.

"Toutefois des expériences préalables, exécutées directement sur chaque tourillon avec un comparateur à niveau très sensible, ont confirmé nos conclusions pour le tourillon *ouest* et donné des résultats très satisfaisants pour la cylindricité du tourillon *est*.

"De plus, l'inclinaison mesurée avec le niveau restant la même dans toutes les directions de la lunette, on peut conclure que ce tourillon *est* se trouve également cylindrique."

This, then, is the upshot of the investigation which fills pp. C. 78 to 93 of the *Étude*! In the case of the Bischoffsheim circle such an investigation has not yet been made, the reason being given on p. C. 120:

"Ici, comme pour le grand méridien, devait se placer l'étude de la forme du tourillon percé. Mais . . . il s'est produit, dans la taille de la lentille tournée vers le tourillon, un petit effet de cylindricité qui ne nous a pas permis de voir simultanément les fils verticaux et horizontaux et, par suite, de procéder à la recherche de la forme de ce tourillon. Il eût faillu, pour l'entreprendre, préalablement retoucher la partie latérale du disque de verre. Or, le beau temps étant survenu, nous n'avons pas voulu suspendre plus longtemps les travaux d'observations, et nous avons remis cette étude complémentaire à une époque ultérieure plus convenable."

From the "*du* tourillon percé" it seems that also in the case of the new Bischoffsheim instrument only one of the two pivots is perforated, so that, at best, M. Loewy's variation of the old method could not be applied except to that pivot.

But is there any well-founded reason against the application of the old method itself? Perhaps even the microscope-micrometers which were used by M. Villarceau in his "Étude du Mouvement de Rotation de la Lunette Méridienne," published 1863, in vol. vii. p. 307 ff. of the *Annales de l'Observatoire de Paris*, may be still available, so that it would only be required to mount and fix them properly, and to have the dots made, in order to be ready for the examination of both pivots. Or is there anything in which the old method is not superior to M. Loewy's variation? In simplicity, in trustworthiness, in applicability—is the comparison not all in its favour?

But then, the old method is totally ignored in what M. Loewy says on p. C. 2 of the *Étude*:

"A l'aide du nouvel appareil, il nous a encore été permis de résoudre un problème aussi intéressant au point de vue de la Mécanique qu'au point de vue de l'Astronomie. Il s'agissait de rechercher si la forme des tourillons est rigoureusement cylindrique. La seule méthode connue jusqu'ici pour résoudre cette question est d'une application difficile: elle exige l'installation de piliers et de collimateurs spéciaux, aussi n'a-t-elle été essayée qu'exceptionellement dans un observatoire ou deux."

How is this ignoring of a well known method to be accounted for?

When, in the spring of 1880, Prof. Houzeau was in London, and I had the pleasure of meeting him at a friend's, he suggested, as a method for the determination of the flexure of a telescope the fixing of a plane mirror in front of the middle of the object-glass and the observation of the reflected image of the webs in different positions of the telescope. It was pointed out in reply that that would not do for determining the *astronomical* flexure, for he would find, on examination, that in that way he would far more likely get the sum of the flexures of the two halves of the telescope than their difference, which he wanted to get. I remembered the unfavourable circumstances which had prevented proper discussion, when I read in the *English Mechanic* of Feb. 24, 1882, vol xxxiv. p. 587, an article, "Measuring the Flexure of Telescopes," from which it appeared that Prof. Houzeau had brought his suggestion before the Belgian Academy of Sciences. I find that Houzeau's paper, "Sur un moyen de mesurer la flexion des lunettes," was brought before the Academy on Oct. 8, 1881, and that from the article in the *English Mechanic* readers may learn all that is required.

What is practically the same suggestion has since been published by Mr. Schaeberle in the *American Journal of Science* for May 1882, p. 374.

In my method of finding the astronomical flexure, the flexure of the eye-end half of the telescope tube is determined by observations of the image of the webs reflected from a plane mirror before an object-glass. Why not use the same means to determine the flexure of the whole tube? The thought is so simple and, for a few moments, so tempting. The equations which show what the micrometer readings R represent are, in the case of the

$$\text{half tube, } \nu - \rho - q = R,$$

and in that of the

$$\text{whole tube, } \nu - \rho - o = R,$$

and therefore apparently analogous. But there is this great difference, that in the first case ρ can be determined, and in the second case it cannot and remains mixed up with the astronomical flexure. What is the value of ρ likely to be? If the axis of the object-glass retains the same relation to the axis of the object-end half of the telescope tube, it will always pass through a certain point near the centre of the instrument. If it does so, the value of ρ will be $= -2o$, and the equation for R becomes $r + o = R$, or the observation of the reflected image gives the sum and not the difference of the flexures of the two ends of the telescope. But, of course, this is merely a conjecture, though perhaps a plausible conjecture. The astronomical flexure is not affected by any rotatory motion of the object-glass round its

optical centre, but merely by the motion of translation of this centre, while the observations of the image of the webs reflected from a mirror attached to the front of the object-glass are affected by any wabbling of the latter, and may therefore show many variations, which will not be of any importance for the astronomical observations.

At the time of my conversation with Prof. Houzeau I was not aware that the suggestion had ever been carried out in practice, and it is only in hunting up in the *Comptes Rendus* some of Porro's papers that I have since come upon the track of Secchi's experiments. Prof. Houzeau says in his paper: "Heureusement il existe un moyen . . . auquel on s'étonne que l'on n'ait pas songé auparavant." But it turns out that Secchi not only thought of it long ago, but practically tried it. His paper, "*Sulla maniera di determinare la flessione dei tubi nei grandi strumenti astronomici*," which I know only by its title (to be found also on p. 900 of the *Bibliographie de l'Astronomie*, par Houzeau et Lancaster), has been published in Tortolini's *Annali*, v., 1854, and in the *Corrispondenza Scientifica in Roma*, iii., 1855. In vol. xxxviii. p. 735 of the *Comptes Rendus* Porro refers to an account of Secchi's paper in Moigno's publication *Cosmos* of 1854. As neither Secchi's paper nor the fourth volume of *Cosmos* are in the library of the Royal Astronomical Society, and as probably not a few readers will like to learn something about Secchi's experiments, it seems worth while to reprint the essential part of Moigno's statement. I, for one, should have been glad to have been acquainted with it long ago.

"Il [Secchi] a fixé à la partie de sa lunette qui portait l'objectif, et au devant de l'objectif lui-même, un petit miroir plan qui venait d'un sextant de Munich, et il a éclairé obliquement les fils du réticule oculaire . . . En procédant de cette manière, le P. Secchi s'est aperçu que les flexions étaient loin de suivre la loi des sinus des distances zénithales. Il y eut même de telles anomalies dans les résultats obtenus des deux côtés du zénith, les observations au nord étaient tellement en désaccord avec les observations du côté sud, que le P. Secchi resta quelque temps dans la plus grande perplexité sur l'origine de ces divergences.

"Enfin il s'aperçut qu'il suffisait de laisser la lunette quelque temps en repos dans une position non verticale, dans la position horizontale, par exemple, pour que sa flexion, nulle d'abord, dépassât peu à peu un angle de deux secondes. La lenteur avec laquelle s'accomplissait la flexion, et le temps qu'elle exigeait pour disparaître étaient les causes véritables de toutes les anomalies constatées. Mais comment tenir compte dans les observations d'éléments si peu susceptibles de mesures? L'habile astronome propose d'adapter pour cela à toutes les lunettes les appareils propres à éliminer l'erreur de collimation, à se mettre à l'abri de la flexion des tuyaux télescopiques. Ces appareils pourront consister, soit dans les objectifs de M. Porro, soit dans

le petit miroir plan dont nous venons de parler, soit dans un miroir concave fixé contre l'objectif, soit enfin dans une lentille intermédiaire qui se placerait de façon à donner sur le réseau oculaire l'image d'un réticule appliqué contre la lentille objective. Cette dernière méthode serait préférable au dire du savant astronome, car elle permettrait d'éliminer les flexions et pendant le jour et pendant la nuit, tandis que le peu de lumière réfléchie par les fils ne permet guère d'observer leur image renvoyée par le miroir quand les rayons du jour pénètrent librement dans la lunette."

Porro's object-glass, spoken of by Secchi, is, so far as I know, a merely theoretical one in which the radius of curvature of the fourth surface is made equal to the focal length, so that the image of the webs may be reflected from this surface itself (vide *Comptes Rendus*, vol. xxxvii. p. 752). In his note (vol. xxxviii. p. 735), already quoted, Porro says of the result of Secchi's mode of measuring, ". . . la mesure ainsi prise du déplacement de l'image du fil ne représentera pas la flexion, ni même une fonction mathématiquement assignable de cette quantité . . ." Secchi, I presume, has acquiesced in the justice of Porro's remark, as upon consideration, no doubt, Prof. Houzeau and Mr. Schaeberle will do.

But though measurements of the reflected image procured by means of a mirror attached to the object-glass cannot lead to the determination of the astronomical flexure of a telescope, they will furnish evidence of the real motions of the axis of the object-glass, the study of which will be of interest to the astronomer as well as to the mechanic, and it seems to me, therefore, that in the case of very large object-glasses it would be worth while to make such measurements.